Salmon's Argument for the Inductive Nature of Falsification and Corroboration in Popper

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1. Introduction

Salmon [1966, 1974] claims that Popper [1959] is really an inductivist because he provides an account of science in terms of nondeductive or "ampliative" inference, by which he means simply any inference which adds information content not already contained in the premises [Salmon 1966, pp. 8-9]. Popper claims, on the other hand, that his method of corroboration through attempted falsification is fundamentally different from induction, and cannot be subsumed under it as some kind of variation, as Salmon would seem to have it. Salmon, building on the work of Reichenbach [1949], tries to save induction, while Popper tries to show that it is just fundamentally flawed.

The challenge to induction was instigated by Hume [1740], who argued that there is no way to justify generalizing past instances into a universal law, as a naïve view of science would seem to suggest. No matter how often I see the sun rise in the morning, there is no reason to presume that this implies that it must rise tomorrow. There have been attempts, like those of Reichenbach and Salmon, to save induction by developing a probabilistic inductive logic that does not generalize with certainty, but that is justified in placing a high probability on, for instance, the sun rising tomorrow after some number of instances have been observed. There is no general agreement that they have been successful.

Popper claimed that scientists do something fundamentally different from collecting positive instances of a law and then generalizing to the law. Rather, Popper claimed that scientists conjecture theories to explain observations, which they then put to the test by attempting to falsify the theories, rather than by attempting to confirm them. Science is thus a negative not positive enterprise; it works by eliminating possibilities, not by generalizing from instances to universals. True, we take some theories more seriously than others because of the evidence, but this is due to the fact that they have withstood more severe tests than the others, and cannot be considered an inductive generalization to some kind of higher probability for that theory. Popper calls such severely tested theories "highly corroborated" rather than "highly confirmed", and does not believe the process yields a probability value for the truth of the theory, as it does in most inductive logics.

In Salmon's view, however, if Popper is to have a positive view of science as truth-seeking (which Popper would, I think, want) then he must somehow escape from Hume's challenge to induction. But, says Salmon, Popper cannot do this by claiming he is doing something other than induction, at least not without becoming, like Hume, a skeptic. Hume said that we simply cannot justify inductive inferences rationally, and we thus are forced to simply accept them as a brute fact of our instinctual natures. But Salmon does not really view Popper as a Humean skeptic, and in fact is quite sympathetic to Popper's system. He is also, however, committed to justifying induction. He thus attempts a reconciliation of falsification and induction, constructing a sophisticated statistical argument to show that, counter to what Popper claims, falsification and corroboration are really just induction in disguise.

To defend Popper against Salmon would require an argument to show that the steps in Popper's method that produce his "corroboration" measure cannot be regarded as inferences. To do this, Popper must deny that we are drawing conclusions, or that we think our results are strengthened by the corroboration. It would seem hard to defend this, given that Popper does allow that certain theories are better corroborated than others. And how can he really avoid the general good advice to take these theories more seriously? Yet Popper does seem to say, at least with respect to the scientific method, that we do not get from science good reasons to prefer a theory—that this is not what science is all about.

In section 2, I will briefly survey Popper's view of science in terms of falsification, and in section 3, I will look at Salmon's view of science as induction. Finally, in section 4, I will look at Salmon's attempt to subsume Popper's falsification under the banner of induction, and discuss its merits and weaknesses.

2. Popper's Falsification and Corroboration

If Popper was straightforwardly a skeptic like Hume, then Salmon would, of course, have no issue with him. The problem is that Popper does not see his method simply as a matter of describing how scientists actually do their work—a sociology of science, perhaps. He really does see it as a quest for truth, as in some sense a positive enterprise, even if an imperfect one. If, however, he were left with no reason to give any greater credence to better-corroborated theories, then he would be nothing more than a conventionalist, something he is determined to avoid. But "the only way to avoid conventionalism," he says, "is by taking a *decision*: the decision not to apply its methods. We decide that if our system is threatened we will never save it by any kind of *conventionalist stratagem* [Popper 1959, p. 82]." So anti-conventionalism is not something Popper feels the need to refute—he finds it unacceptable on first principles.

To understand why Popper does not feel this forces him to be an inductivist, we will need to look a little more carefully at his system. As Salmon claims, Popper's system is ampliative—the information content of the system increases as we progress: "…the theory should allow us to deduce… more *empirical* singular statements than we can deduce from the initial conditions alone [Popper, p. 85].” For instance, it allows prediction. So far, this is the same as inductive generalization.

But Popper draws a very important line between theories that are scientific and those that are not. Those that are not may still be meaningful in some nonscientific sense—for example they may be metaphysical and so may even help drive scientific discovery—but they are not scientific in the sense that they can be subjected to scientific testing. A scientific theory is always in principle falsifiable, and becomes actually falsified “only if we have accepted basic statements which contradict it [Popper 1959, p. 86].” By “basic statement,” Popper means the sort of uncontroversial observation statements that go into reporting the results of experiments (such as “the dial read 0.56 pounds per square inch,” etc.). Such basic statements are the empirical fuel for both falsification and induction.

Popper distinguishes between two types of basic statements with respect to a given theory—those that would, if accepted, contradict the theory, and those that would not. The former are the potential falsifiers of the theory, and “a theory is falsifiable if the class of its potential falsifiers is not empty”, otherwise it is unfalsifiable and not scientific. An example of an unfalsifiable theory might be the idea that God created the world three seconds ago, merely setting everything up (being omnipotent) *as if* it had been around for billions of years, including implanting our past memories into our newly formed heads. The theory sounds absurd, but there is no way you could possibly prove it wrong, no matter what evidence you came up with. It is thus not a candidate for a scientific theory. Some theories that do not seem to be inherently unfalsifiable are effectively made so by defenders who take the attitude that the theory will be modified or fleshed out *ad hoc*, as needed, in order to save it at all costs no matter what evidence seems to speak against it. Flat-earthers who come up with convoluted hypotheses in order to explain their way around the evidence for a round earth—even postulating government cover-ups of faked moon landings—are acting outside the realm of science, since they have taken a theory (that the Earth is flat) that was once perfectly respectable, and in order to save it from falsification have developed it into an unfalsifiable theory.

Popper does not simply divide theories, however, into “falsifiable” and “unfalsifiable”. Indeed, some theories may be largely unfalsifiable, but leave some small room for falsifiability, while others may be easily falsified in principle (“easily falsified” does not mean they actually can be readily falsified—which would mean that they were false—it just means that it is easy to come up with possible experiments which we can imagine might falsify them). Popper considers it perfectly correct, then, to speak of degrees of falsifiability, with tautology and metaphysics on one end (completely unfalsifiable) and contradictions on the other (*a priori* falsifiable). In a sense, the higher falsifiability corresponds to higher information content, and as Shannon [1949] showed, high information content correlates with *low* probability, not with high probability. So highly falsifiable theories are those which would be considered *a priori* to be less probable [Popper, p. 140].

Modern scientists have largely accepted Popper’s demarcation between scientific and nonscientific statements, and they routinely apply Popper’s criterion to theories to evaluate their inherent value (if you can show that someone’s theory is unfalsifiable, it will generally be considered dead out of the water).

Because Popper’s system is ampliative, to use Salmon’s term, its information content must increase as we apply its methods. Yet how can it do this without inductive generalization? Because, says Popper, the information increase occurs in the elimination of false theories, not in the generalizing to true (or probably true) ones. A falsifying experiment decreases the range of "possible worlds of experience" [Popper, p. 90]. It does *not* tell you *which* possible world you are in (by the production of universal laws, for instance), rather it tells you which possible worlds you are definitely *not* in. Rather than going from a large number of singular statements to a single universal law, Popper goes from possibly only one singular statement to the ruling out of a universal law and hence an infinite number of singular statements (its instances).

Of course, in reality, it takes more than one falsifying singular statement to really falsify a theory. However, in principle, according to the logic of Popper's system, it takes only one. All I need is *one* example of the law of gravity being violating, and I can categorically state that the law of gravity is not a universal law. Sure, there may be practical problems with accepting only one isolated falsifying case—the scientist involved might have made mistakes, even lied or hallucinated. So in practice, we require a falsification to be *reproducible* by different scientists before we accept it. Nonetheless, the logic of the system only requires one falsification. Reproducibility is thus a "material" rather than formal requirement of Popper's system. Formally, a falsification need only be a singular existential statement (such as "there is a such-and-such here at space-time location (x,y,z,t)."). In fact, this is Popper's basic formal requirement for a basic statement: "*basic statements have the form of singular existential statements* [Popper, p. 101-102]." Material requirements are added to ensure that the basic statement is in practice observable. The effect must not only be observable, it must be inter-subjectively observable, so that a community of scientists may come to some agreement on it. Another material requirement is that we embed our observations in some kind of theoretical framework—that "we should not accept *stray basic statements*—*i.e.* logically disconnected ones—but that we should accept basic statements in the course of testing *theories* [Popper, p. 106]."

Material requirements are required in practice before we can even deign to accept a basic statement as a falsification, but so long as we do in the end, as a scientific community, accept at least one such case, the universal law in question is finished (although it may still serve some practical purpose if it continues to work most of the time, or as an approximation method).

The problem with induction is that it would require, in principle, the observation of every possible instance (obviously impossible, since there are an infinite number of them) before any conclusion could be logically accepted. Popper gets around this by requiring (logically) only a single instance before a conclusion is drawn. Not only that, but his conclusion is deductive, since it simply follows *modus tollens*:

Let $T(x)$ be a proposed universal law of nature—a theory—that takes initial conditions x as a parameter. T by itself returns *true* if the theory is true, while $T(x)$ returns some conclusion or prediction that follows deductively from T and initial conditions x:

$$T \;\&\; x \rightarrow T(x).$$

We say that T is scientific—falsifiable—if and only if there exists at least one testable statement B, which if true would imply that $T(x)$ was *false*. Let $F(T)$ be a predicate that tells us whether T is falsifiable:

$F(T) \Leftrightarrow \exists B\, \exists x\, [\, B \rightarrow {\sim}T(x)\,]$ T *is falsifiable iff it has a potential falsifier.*

A falsification is driven by the logic of *modus tollens*:

$T \;\&\; x \rightarrow T(x)$	*the theory makes a prediction, given initial conditions x.*
x	*the initial conditions x hold.*
${\sim}T(x)$	*the prediction is falsified.*
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${\sim}T$	*the theory is falsified.*

This is the logical engine of Popper's system, and it is deductive, unlike an inductive system which tends instead to add some degree of confirmation to T for every T(*x*) that is found, increasing the probability of T, call it p(T), with each observation. The inductivist reasons that, if we were to have infinite resources and could examine all possible T(x), then we could *deductively* conclude T from our observations. We cannot, of course, observe all the infinite number of possible instances of a law, but the more instances we do observe, the more likely it is that we have a correct law. The idea here is that somehow p(T) will converge, *in the limit* of infinite observations, on 100% if the theory is true, even though we can never actually get to 100%, since we can only make finite observations. We also cannot ever know that we have converged at all, or even that we are "close" within certain limits. Nonetheless, if we can show that convergence *will* occur eventually, given unlimited time, then we can claim some justification for the procedure, even if we can never actually recognize when convergence has taken place.

Popper, of course, might agree, in a flight of fancy, that if somehow a God could observe all possible instances, then the above would be valid. But since we are not gods, we cannot, and Popper does not believe the computation of non-limit values of p(T, *n*) for finite *n* makes any sense. The probability of a theory might, according to our inductive function p(), be 99% after fifteen billion observed instances, without the theory's being true at all. This is Hume's problem of induction. No number of observed instances rationally justifies the leap. So Popper claims he does not need the method at all. We are not trying to come closer and closer to some infinite sequence of observations. Rather, we are seeking to falsify our theories, and move forward by eliminating possible worlds of experience, and that is a deductive process (in terms of its logic, of course, not its entire practical application).

Let us return now to the unfalsifiable statements. There are many types. They can be about experience, in which case they are metaphysical rather than scientific in nature, or they might be completely mathematical or logical, in which case they are tautologies (or perhaps "tautologies and mathematical truths" if one's philosophy of mathematics classifies these two as different—I will, however, proceed as if mathematical truths are a kind of tautology). A tautology is necessarily true, so obviously it is also unfalsifiable. This is as it should be, since mathematics and logic are not part of science (or at least not "empirical science", which is the kind of science Popper and Salmon are both concerned with). Metaphysical statements are the more interesting case, since they seem to have empirical content, being about possible experience. Some metaphysical statements might turn out to be tautologies in disguise, but others might be theories that are just too vague to be falsifiable, others might be meaningless or incoherent and unfalsifiable on those grounds. There are many reasons a statement might lack potential falsifiers, and Popper is not eliminating all unfalsifiable statements from science. Just as tautologies can be useful in science, in the process of formulating the theory and its deductive consequences in the first place, likewise metaphysical statements can drive scientific discoveries, as they can guide the process of formulating new scientific theories. They cannot actually become mature, testable scientific theories, however, until we can say just what it would take to convince us that they were false. On the other end of the spectrum are self-contradictory statements, which are automatically falsifiable, since from a contradiction, one can derive anything. "Nonfalsifiable statements assert, as it were, *too little* about the class of possible basic statements, self-contradictory statements assert *too much* [Popper, p. 91]." An empirical theory thus has two *a priori* requirements, that it be consistent (required for any system, empirical or not) and that it be falsifiable (required only for empirical systems) [Popper, p. 92].

It is important to realize that Popper's system is not meant to be entirely deductive in application. The basic logical engine that drives it is deductive, but there are many pragmatic, real-world issues concerning its application that will require estimation, guesswork and even induction. Popper is trying to avoid total skepticism without sinking into absolutism. He does not see science as being about certainty, even though it may be about the search for truth. He thus separates off the deductive element, to show where the purely logical and rational core of science lies, which is not meant to imply that there is no vague and fuzzy work around the edges of all this that is just as necessary for scientific progress. Popper does not address the issue, for instance, of where theories come from—that is a matter of creativity, and often involves metaphysics. He addresses only how theories are *tested*.

For instance, one of the key steps in Popper's method is to come to an acceptance of some basic statement or other. Yet, as Popper stresses, there is no such thing as an absolutely basic statement. No statement can really be 100% singular. Every observation is theory-laden and assumes some background knowledge prior to its acceptance. This must be the case, since there are an infinite number of ways to categorize the world around us and hence an infinite number of possible basic statements to make. Our choice of basic statements reflects a theoretical bias. To say "I observe a blue chair here" may sound completely "basic", but who said you were to classify the world in terms of chairs in the first place, or that you were divide the colour spectrum up in such a way as to get "blue"? [Popper, p. 421]

Any basic statement assumes some theoretical language—often called a "basis language"—within which the observations are framed. It is always possible that a later falsification will require us to modify our basis language (this happened arguably in a big way when the wave-particle duality of energy and matter was discovered). Certain questions and statements that were expressible in the old basis language then become meaningless, or at least in need of reformulation, in the new basis language. The collecting of basic observations in science is itself theoretically driven. "Thus the real situation is quite different from the one visualized by the naïve empiricist, or the believer in inductive logic. He thinks we begin by collecting and arranging our experiences, and so ascend the ladder of science. Or, to use the more formal mode of speech, that if we wish to build up a science, we have first to collect protocol sentences. But if I am ordered: 'Record what you are now experiencing' I shall hardly know how to obey this ambiguous order. … A science needs a point of view, and theoretical problems… The inductive logician… is prevented from explaining regularity by theories, because he is committed to the view that theories are nothing but statements of regular coincidences [Popper, p. 106]."

So, in this respect—on the acceptance of basic statements—Popper is a conventionalist. "Basic statements are accepted as the result of decision or agreement; and to that extent they are conventions [Popper, p. 106]." The *modus tollens* falsification engine works, therefore, *given* some set of background information accepted *by convention*. The key here is that there is a rational core to the progress of science, not that it is entirely rational from head to toe. In the case of an inductive logic, there is no rational core at all, given Hume's argument (of course, someone like Salmon will want to say that inductive logic *can* be made rational.

Our observations, while theory-laden, may or may not be based on actual lower-level corroborated theories, although they might be. However, it is also possible they might be based on simple common sense notions or folk theories (although obviously ones that the scientific community can accept as reasonable assumptions in the context of the current experiment). So induction and other nondeductive methods may in fact be involved in this aspect of science.

Salmon will, of course, try to argue that Popper nonetheless will give better tested theories a higher degree of corroboration than theories that are less well tested, and thus he is to some extent inferring a higher probability for those theories, and is performing a kind of induction. But Popper would disagree. The logic of scientific testing merely eliminates theories. We accept a theory as better corroborated ultimately by convention. The choice of the better theory as a working hypothesis is not the result of a scientific, rationally justifiable inference, but is a convention that belongs to the creative and intuitive side of science, along with the metaphysics involved in theory formation and in the background knowledge for testing. So long as this "inductive" aspect is kept out of things, the logic by which science proceeds is not itself inductive. The fact that more severely tested theories are the ones that are left for future experimenting is a natural result of the falsification process, but that does not mean that the *conclusion* that one theory is better corroborated can be said to be justified, any more than the folk knowledge that tells us it is okay to talk about blue chairs can be said to be justified when we use it as a basis language in describing our experimental observations.

Popper asks, "How and why do we accept one theory in preference to others? …certainly not due to anything like an experiential justification… not due to a logical reduction of the theory to experience. We choose the theory which best holds its own in competition with other theories; the one which, by natural selection, proves itself the fittest to survive. This will be the one which not only has hitherto stood up to the severest tests, but the one which is also testable in the most rigorous way. … it is *decisions* which settle the fate of theories. To this extent my answer to the question, 'how do we select a theory?' resembles that given by the conventionalist; and like him I say that this choice is in part determined by considerations of utility [Popper, p. 108]." But what distinguishes Popper from the conventionalist is that "the convention or decision does not immediately determine our acceptance of *universal* statements but… enters into our acceptance of the *singular* statements—that is, the basic statements [Popper, p. 109].” Once these singular statements are accepted, even if by convention, they are fed into the *modus tollens* engine, and *that* is completely deductive.

This separation of the conventionalist and the deductive aspects of science puts Popper in the interesting position of supporting an actual mathematical measure for “degree of corroboration” that ends up reading much like a probability for the truth of a theory, even though he steadfastly insists that there is no degree of justification, or probability of truth, involved.

> “(1) A statement *x* is said to be ‘falsifiable in a higher degree’ or ‘better testable’ than a statement *y* … if and only if the class of potential falsifiers of *x* includes the class of potential falsifiers of *y* as a *proper subclass*.”
>
> (2) If the classes of the potential falsifiers of the two statements *x* and *y* are identical, then they have the same degree of falsifiability…
>
> (3) If neither of the classes of potential falsifiers of the two statements includes the other as a proper subclass, then the two statements have non-comparable degrees of falsifiability [Popper, pp. 115].”

In other words, better-corroborated theories are the ones that provide more opportunities for falsification—that are, in some sense, *more* falsifiable. “The classes of potential falsifiers of all tautological and metaphysical statements are empty,” giving them a “zero degree of falsifiability… A self-contradictory statement… may be said to have the class of all logically possible basic statements as its class of potential falsifiers [Popper, p. 116].”

Popper is not religious about the proper subclass quantification scheme, and is open to others, so long as the general idea is that higher falsifiability, by some measure or other, means higher corroboration, given the same degree of success at surviving tests. In fact, Popper notes that the subclass scheme does not really apply to theories with disjoint sets of potential falsifiers, and he even provides a second method of quantification based on the dimensionality of the space of potential falsifiers—theories of a lower dimensionality are more easily falsified [Popper, pp. 129-133]. The subclass measure, however, will do for our current purposes. The basic point is that Popper *does* see corroboration as quantifiable, like induction, and yet denies that it *is* induction in any form.

Not only does Popper accept a quantification of falsifiability and hence corroboration, but he even feels that it is in some sense related to probability, since highly falsifiable theories—the ones that risk more in being tested—are the ones with *a priori* lower probability, and higher information content—they tell us more about the world. To Popper this is still not induction, however, since an inductivist seeks higher probability by repeated observations of the tested theory, while a Popperian scientist seeks to test theories that are of lower probability, and simply leaves his knowledge at that. It is Salmon's goal to show that this use of what is clearly a probability measure amounts to a kind of induction after all.

3. Salmon's Justification of Induction

One of the main problems in supporting induction against Hume is that one must use induction to do it.

"Why," asks the skeptic, "should I believe that a pattern will continue to repeat just because it has in the past?"

"Because," replies the inductivist, "this method seems to have worked pretty well for us in the past—look at the great success of modern science!"

Of course, one immediately sees the problem—this inductivist has used induction to support induction. How can one justify one's method with the self-same method? To use a delightful example from Salmon [1966, pp. 12-13], a crystal ball gazer might do the same by gazing into her ball and declaring that it tells her that ball-gazing is the best way to truth, and yet somehow we would not take *her* seriously. Yet we seem to feel it is okay to justify induction by using induction. We are even in trouble if we try to argue that, after all, the ball-gazer's methods have really not worked so well in the past, giving us some prior reason to dismiss them. The problem is that we are then judging ball-gazing by the standards of the inductive method. If we can do this, why should the ball-gazer not agree with us that her method has not worked in the past, but point out that her crystal ball tells her that her predictions *will* work from now on, and that "the scientific method is in for a very bad run of luck"!

Obviously, inductivists need a more coherent justification for their use of a "self-supporting" argument if they are to have more credibility than a fortune teller. Salmon [1966, pp. 13-15] suggests three requirements for a self-supporting argument:

(1) The argument must have true premises.
(2) The argument must conform to a certain rule.
(3) The conclusion of that argument must say something about the success or reliability of that rule in unexamined instances of its application.

Supports of induction using induction have these characteristics, but so do deductive arguments that seem in fact to have no real content whatsoever. For instance: "If snow is white then *modus ponens* is valid. Snow is white. Ergo, *modus ponens* is valid." This is obviously totally empty, yet somehow we still feel we can use a completely analogous argument to justify induction. Yet we could even use such an argument to justify an invalid method. Yet Salmon [1966], following Black, objects that this comparison is unfair, since "we know on independent grounds that such rules are faulty." Salmon's tentative answer at this point is based on the fact that we have some "independent" initial trust in induction that is enough to get the argument off the ground, and the self-supporting nature of it then allows us to "jack up" our faith in the method from there. This makes some intuitive sense, probably because induction is so psychologically compelling in the first place, but really it is dubious in the extreme as a rational argument. Why not use this method to justify crystal ball gazing, so long as we have "some degree of faith" initially in the method? (Indeed, believers in New Age, occult and other mystical phenomena often do just this to "jack up" their belief in their methods, and then use these methods to create what is essentially a self-reinforcing delusion. Few in the scientific or philosophical communities take such people seriously, so why should we treat self-supporting arguments for induction any differently?

Salmon, in fact, is not really satisfied with this justification (although he seems somewhat compelled by it), saying that still "Hume showed that inductive justifications of induction are fallacious and no one has since proved him wrong." Salmon presents a far more ambitious attempt to save induction, following the lead of Reichenbach [1949]. Reichenbach did not claim to firmly establish the validity of his inductive method, but did claim that "if there is any method of inference whatever which fulfils that knowledge-extending function, then his rule of induction will do so also [Salmon 1974, p. 85]."

Hume said we cannot assume that nature is uniform. According to Reichenbach, defending induction amounts to justifying the claim that nature *is* uniform. He presents something like Pascal's wager on the existence of God; we will call it "Reichenbach's Wager". If we decide to use induction, then either it is valid because nature is uniform, in which case things work out fine, or it is invalid because nature is not uniform, in which case we fail. If we do *not* use induction, however, then the odds are inherently worse, since any noninductive method will also fail if nature is not uniform (one must presume), but if nature *is* uniform, such a method *may or may not* fail. But if we use induction, we are guaranteed success if nature is uniform! So we might as well use induction, unless we have some independent reason for eliminating failure (if nature is uniform) as a possibility for some noninductive method. Until such a method comes along, it is only rational to use induction, even though we cannot absolutely justify it. Its success may depend on the uniformity of nature, but it *will* work if nature is uniform and *nothing* will work if nature is not. It is justified better than any other method out there, in other words—fewer assumptions are required to justify it than its competition.

A problem with this wager is, of course, the tacit assumption that no method better than induction is currently known. Popper might well argue that *his* method is the actual method scientists use, is *not* induction, and works better than induction; so Reichenbach's Wager is not such a good bet anymore. Just as Pascal's Wager relied on the assumption that there was no independent evidence for the non-God hypothesis, Reichenbach's Wager relies on the assumption that there is nothing better out there right now than induction, and Popper would disagree with this heartily!

Another potential problem with the Wager is its assumption that uniformity implies the success of induction. It is not clear to me that Popper would agree with this. Nature might be uniform, but still resist modeling via the accumulation of finite examples. Nature's uniformity, it seems to me, would imply success eventually, at least in a theoretical sense—since if we tried every possible model in sequence and every possible experiment, we would eventually amass more evidence for the correct model than for its competitors. However, this notion of "success" is very mathematical and "in principle" and not remotely practical. We could never know, for example, that our method had "converged". We would only know that it would eventually converge, and it is entirely possible that nature might be uniform, yet still complex enough to defy induction within any vaguely reasonable time frame (and, indeed, I think Popper might hold just this).

Furthermore, Popper does not really see an assumption of uniformity as central to science. Since his system is based on falsification, which is deductive, no such *a priori* assumption is needed: "I abstain from arguing for or against faith in the existence of regularities in our world [Popper, p. 253]."

However, let us allow for now that Salmon should be permitted to defend induction on logical grounds, since Hume attacked it in the first place on such grounds. Reichenbach considers that the kind of induction he is defending is induction by enumeration, which is based on convergence to an infinite limit of relative frequencies [Salmon 1974, p.87]. So "success" becomes "value of the limit is established", while "failure" becomes "value of the limit is not established" and "nature is uniform" becomes "the sequence has a limit" [Salmon 1974, p.87]. Salmon's most serious objection to Reichenbach's justification of induction (which he says Reichenbach was aware of) is that "there is an infinite class of rules—called 'asymptotic rules'—which are equally justified by the same argument [Salmon 1974, p. 88-89].

Salmon formalizes the idea of the limit of a relative frequency as follows:

$f_n = m/n \rightarrow lim_{n\rightarrow\infty} f_n = m/n + c$

Where *m* is the number of observed confirming cases or "hits", *n* is the total number of observations and f_n is the frequency after *n* observations. This rule declares that the observed frequency over a finite time will converge in the limit of infinite time on the correct value.

The final term *c* is a "corrective" term, which measures the degree to which the finite case differs from the limit value. It is not necessarily a constant, but could also be a function of arbitrary complexity. Any rule where $c\rightarrow 0$ as $n\rightarrow\infty$ has very similar properties to induction by enumeration, and also produces the correct value in the limit, and so is known as an "asymptotic rule". Induction by enumeration is the asymptotic rule that results when we set c=0.

So why should we prefer one asymptotic rule (c=0) over others that also produce the correct limit value? The question at this point is whether there are any reasonable rules that are *not* induction by enumeration, yet still asymptotic. For many nonzero values of c, the rule is nonasymptotic, and so does not satisfy Reichenbach's Wager in the first place (*e.g.* any purely *a priori* rule, or a "counter-inductive" rule that follows the *opposite* of what past experience suggests). But Salmon gives one example that *is* asymptotic and also *not* induction by enumeration (it has a nonzero c) and thus that presents a problem for Reichenbach:

$$f_n = m/n \rightarrow \mathit{lim}_{n\rightarrow\infty} f_n = [1/(n+1)](m + 1/k)$$

This is called the "vanishing compromise" rule [Salmon 1974, p. 88-91]. The variables m and n are defined as before, while *k* represents the number of categories, or mutually exclusive predicates that are used. The rule gets its name from the fact that it combines "an empirical factor with an *a priori* factor, but… the *a priori* factor carries less weight as the amount of empirical evidence increases." In the limit, then, it corresponds with induction by enumeration. So what is wrong with this? It is essentially standard induction along with some *a priori* assumption that carries less and less weight, the more evidence is gathered [Salmon 1974, p. 90]. This sounds pretty reasonable. In fact, the *a priori* assumption represented by the term with *k* in it is something like Popper's guiding background knowledge. Both are allowed to be metaphysical or conventionalist in nature.

Indeed, Salmon considers the vanishing compromise rule to be a worthy candidate (even though he will eventually dispense with it). It has both the desirable property of being asymptotic, as well as the other feature Salmon insists on, "regularity" (which just means that the frequencies have been normalized and thus form a proper probability or relative frequency distribution, where the sum of all the relative frequencies must be one).

Of course, Salmon is not suggesting this is the only possible rule that satisfies both asymptoticity and regularity. Other rules might have a more complex *a priori* (metaphysical) assumption built into them. So the whole idea has a very plausible ring. Metaphysical assumptions and background knowledge are taken into account, but it is empirical evidence that has the final say at the end of the day.

In fact, however, we can choose for *any* arbitrary value of the limit and any possible sample set, an appropriate *a priori* assumption that will give us a regular, asymptotic rule that will justify that particular limit value. Salmon, in fact, has proved that this is the case [Salmon 1957].

Perhaps this is as it should be. The *a priori* part, it would seem, must be justified on *a priori* or metaphysical grounds. It might correspond to conditions such as cause and effect or locality. Appropriate empirical counter-evidence would eventually swamp such considerations (as has happened with the "locality: and "cause and effect" metaphysical conditions given the advent of quantum theory). The problem, given Salmon's proof, is how to justify the use of such a rule if *any* result can be maintained given some appropriate metaphysical assumptions. Can we constrain the metaphysics in such a way that the inductive method still has some justification a la Reichenbach?

In order to do this, we must *a priori* constrain the c() function, so that Salmon's proof will have more bite. This constraint cannot be *a posteriori*, of course, since the purpose is to lend the method justification against Salmon's proof that we can rig the system any way we want with an appropriate choice for c(). So Salmon suggests (and this is his primary improvement on

Reichenbach and the key thing that makes his system potentially reconcilable with Popper's) a new criterion based on linguistic invariance. The rule must be *a priori* and justifiable, so Salmon suggests a rule to exclude contradiction. Since the rule is based on some particular analysis in some basis language, it could contain all kinds of strange and artificial artifacts of *that* language. For instance, Salmon [1974, p. 92] gives the example of *k* as the number of color predicates for a random marble pick. If there are three colors of marble, then the *a priori* guess built into the rule will be 1/3 for the probability of picking say a red marble. This is, of course, an artifact of a basis language that just happens to split the colour spectrum into three classes. It might seem that any such analysis ought to be consistent with any other analysis, since they describe the same experiences. That may be true, but we are not warning here against inconsistent basis languages, but against the possibility that the limit *rule* based on one language might be inconsistent with a corresponding rule based on some other basis language. For instance, assume that we change our rule slightly so that "red" versus "blue" becomes instead "red" versus "light blue" versus "dark blue". This is a new and different basis language, but one that is comparable in definite respects with the previous one. For instance, p(red) in both cases refers to the exact same event class. Yet the *a priori* relative frequency of red is now 1/3 instead of 1/4. So the two different languages seem to give contradictory results, even though the *a priori* assumptions within each basis language seemed to make some sense.

So Salmon suggests we make a rule not to allow such inconsistencies across linguistic domains. After all, we are doing science, so we are searching for truth, which ought to be independent of the particular details of the language we choose to describe it in. So any metaphysical assumptions or background knowledge that are taken to precede the empirical inductive process must be objective—*i.e.* they must be applicable over translations to arbitrarily many different linguistic bases. This stipulation of consistency across translation is called by Salmon the "criterion of linguistic invariance":

> "Given two logically equivalent descriptions (in the same or different languages) of a body of evidence, no rule may permit mutually contradictory conclusions to be drawn on the basis of these statements of evidence." [Salmon 1974, p.92]

The vanishing compromise rule violates this criterion because it is dependent on *k*, the number of predicates. Indeed the above rule in the case of the marble pick is not the probabilistically appropriate rule at all, in spite of sounding vaguely reasonable ("I have three colours so the *a priori* probability of any one of them must be 1/3"). If I have two light blue marbles, three dark blue marbles and four red marbles, then p(red) will be 4/9 no matter whether we group the light blues and dark blues together or not. But this correct probability rule divides not by *k* but by the number of possible events, which is the same in either language. Dividing by *k*, as the previous rule did, makes contradictory results possible and so is invalid. "We can hardly suppose it to be a truth of pure reason [and one might also add, a reasonable conention or piece of background knowledge] that there are precisely 347 colours [Salmon 1974, p. 93]."

The invariance criterion ensures a kind of objectivity to our metaphysical or background assumptions. Of course, since there are an infinite number of different languages, and we cannot apply this rule to all of them, we may not be able to actually determine whether our rule meets the criterion. But, we can still use the criterion, given a particular set of languages, to show certain rules to be inconsistent and thus metaphysically inappropriate.

Does this new scheme create a fit (or at least closer fit) with Popper's system? Perhaps Popper is right that straight-out induction is not equivalent to falsification, while Salmon's modified induction (with its place for convention, background knowledge and metaphysics, and its constraints for their introduction) might be more compatible with Popper's views. This would make Popper in a way right that his system is qualitatively different from induction, but it would make Salmon also right that Popper has somewhat overstated his case, since falsification could be placed within a modified relative frequency system. To answer this question, we will need to look at what Salmon makes of Popper's system compared to his own and whether indeed the two are reconcilable. Salmon seems to think that they are, and that Popper, rather than totally destroying induction, has produced something perfectly consistent with it.

Salmon believes that it is exactly the kind of problems that c() introduces, including issues surrounding background and metaphysical knowledge, that "dealt the death blow to the classical interpretation of probability theory," and that the principle of invariance gives us a solution to this problem. "The classical interpretation made use of the principle of indifference, a principle which states that two possible occurrences are equally probable if there is no reason to suppose one will happen rather than the other. This principle gives rise to the Bertrand paradox." The Bertrand paradox is a paradox based on an inconsistency in using details of a basis language in one's background knowledge; it involves speeds and times, rather than colours of the spectrum [Salmon 1974, p. 93-94].

The problem remains whether there actually *are* any metaphysical and background assumptions that meet the invariance requirement—whether there *are* any c() functions that pass. Salmon dashes any hopes we might have had to reconcile with Popper by placing Popper's background knowledge in c(). He proves [Salmon 1961] that in fact *there are no possible c() functions that pass the linguistic invariance criterion other than c=0,* which is the case of induction by enumeration [Salmon 1974, p. 94]. In fact, since only one rule meets this new condition, it can completely replace both of the old rules of asymptoticity and regularity. "The only admissible ‘corrective’ function, *c*, which is a function of observed frequencies alone, is that which is identically zero [Salmon 1974, p. 95].”

This is quite a result. It shows that in some sense *no* metaphysical or background assumptions (if we accept an induction-based framework at all) can possibly be consistent (except for the null assumption of c() = 0, of course). This is because we have assumed here that we are looking for the limit of relative frequencies. Thus, “either we accept the rule of induction by enumeration for purposes of inferring limits of relative frequencies, or we forgo entirely all attempts to infer limits of relative frequencies [Salmon 1974, p. 95].” This agrees with Popper, it would seem, since forgoing such an option is exactly what he proposed! But, of course, this is not the sort of reconciliation with Popper that Salmon is hoping for.

We are now seemingly left with the option of choosing whether we want to view science as being about limits of relative frequencies or not. Indeed, Salmon does not directly answer this problem. He admits that “the relative frequencies of the attributes we deal with may not have limits. But we can be assured that, if such limits do exist, persistent use of induction by enumeration will establish them to any desired degree of accuracy.” Of course, even given this assumption of uniformity (essentially Reichenbach’s Wager), as we said before, we can still argue on Popper’s side that we just don’t have indefinite time, or even a reasonable approximation thereof. Salmon’s proofs give us a mathematical result about *infinite* limits. But in reality, we always have a *finite*, and indeed extremely restricted sample. So, again, science in Popper’s view does not look for limits, but rather the best tested model so far.

Salmon admits this problem with his justification of induction, calling it the problem of the short run: “we deal in practice with finite sequences only” [Salmon 1974, p. 96]. The case of the short run is, in fact, Popper's best response to Salmon. Probabilities and frequencies are just not what we are after in science, since our resources are not infinite.

Salmon provides no conclusive response to this problem, just noting that there are a few possible avenues, none of which have been successful as yet, although he sees no reason to assume they are "hopeless" [Salmon 1974, p. 97]. However, Salmon does not leave Popper here, as an adversary who may or may not be closer to the truth, pending future research. Even though he goes no further in arguing directly for short run induction, he really does not need to—given Reichenbach's Wager—if only he can show that Popper's system is not really an alternative to induction at all, but simply a case of it. He attempts to do just that by arguing that Popper's falsification and corroboration can be couched in Bayesian probabilistic terms that turn it directly into a kind of inductive logic.

4. Salmon's Attempted Reconciliation With Popper

The question still remains, however, whether Popper's system can be unified with Salmon's. This would obviate the need to deal with the short run. We already suggested that perhaps c() is the noninductive metaphysics and/or background knowledge that prevents Popper's system from being a pure inductive logic. However, we then saw that Salmon can prove that only c()=0 (straight, unadulterated induction by enumeration) will "work", in the sense of converging to the correct limit without inconsistency. However, this very notion of "it works" depends on the assumption that we are looking for limits of relative frequencies in the first place, and ignores the short run. If the short run is what prevents Popper from accepting induction, then perhaps the short run justifies the nonzero c()'s and unifies Popper and Salmon.

The problem is: how can we permit Popper's system to use a nonzero (noninductive) c(), while not being totally inconsistent, since Salmon already showed that only c()=0 is consistent?

This problem may not be so troublesome as it appears, however. Popper does of course require consistency in a theory first, prior to use of his method. So does Salmon, which makes straight induction the only option *if* we ignore the problem of the short run and assume limits of frequencies are what we are after. “I think it is altogether impossible,” says Popper [1959, p. 256], “to accept the suggestion that a hypothesis can be taken to be a sequence of statements.” We could only do so if universal statements *were* summaries of an infinite number of observations. But “universal statement do not have this form. Basic statements are never derivable from universal statements alone”. Even if one tries to argue, and could get Popper to agree, that some kind of God-like perspective might be possible from which universal statements could be viewed like this, one has to concede, with Popper, that we can never actually know these absolute frequencies or show them from experience, and we can never even know (via Salmon’s first proof mentioned above) that we have a big enough sample to presume that induction can be applied at all. This means we *need* some background knowledge for the pragmatic purpose of choosing some value. Straight induction is not justifiable with a finite sample. So we might quite feasibly be *better off* using a nonzero c() than straight induction. Nonzero c() in the short run could actually be the best way of approximating to a zero c in the long run! Not only that, but this nonzero c() could represent exactly the background knowledge, etc., that makes Popper’s system so seemingly noninductive in the first place. So perhaps the two systems are not so far from reconciliation of some kind, after all.

Salmon himself has this to say: “If science consists solely of observation statements and deductive inferences, then talk about theories, their falsifiability, and their tests is empty… Science is barren.” In other words, since Popper, in the logical and hence justifiable portion of his system, provides only deduction, his inferences (to the extent that they are justifiable) are nonampliative, and thus have zero content. This is not Popper's view of his own system, of course, since he believes that the greater the set of potential falsifiers, the more informative is the theory, since if it survives, it survives against the odds. And, as we will see shortly, Salmon does not himself think that Popper is this easily dismissed. But nonetheless, Salmon's point here is still applicable, at least at first blow, to Popper's system. According to Salmon, to say that a theory "has not been falsified" is to add absolutely nothing to our observation statements except logical deduction. Likewise if we say the theory has not yet been falsified. Since *modus tollens* is the only inference method, it adds nothing, is nonampliative and hence "barren" [Salmon 1966, p. 24].

There is thus a fundamental problem for Salmon with any system whose only rationally justified procedure is purely deductive, which (again, at least at first blow) seems to be the case with Popper—to the extent that Popper does add something (creativity in coming up with hypotheses, background knowledge and so forth), he is doing something nonampliative, and thus it would be inductive—*if* he claimed any rational justification for it. Hence Salmon's view that, to the extent that Popper's system is justified, it is inductive, and to the extent that it is not inductive, it amounts to Humean skepticism.

However, as I have mentioned, Salmon does not really think that this argument as it stands is enough to lay skepticism and/or induction at Popper's door (although that is indeed what he will eventually do). Salmon admits that Popper does not really leave his statements barren in the above sense, since he adds to falsification the idea of corroboration, which is absolutely key to preventing Popper's system from becoming a kind of pure skepticism. The easier-to-falsify unfalsified theories are the better corroborated ones. This requires bold conjectures which take the most risks, a process that drives science forward. High falsifiability means high information content, which in turn means low probability—theories *a priori* less likely risk more and thus say more. For this reason, Salmon says that Popper's system is "not properly characterized as *deductivism* [Salmon 1966, p. 25]." It is partly inductive because of the probabilistic nature of corroboration. "Corroboration is a nondemonstrative form of inference," and hence inductive in Salmon's sense. "*Modus tollens* without corroboration is empty; *modus tollens* with corroboration is induction [Salmon 1966, p. 26]."

So is corroboration straightforwardly inductive, then? "I do not want to quibble over a word in claiming that Popper is, himself, a kind of inductivist," he says. "The point is not a trivial verbal one. Popper has claimed that scientific inference is exclusively deductive." Salmon claims [1966, p. 26] of course that we have already seen that it is not, even by Popper's own lights.

> "Using the same force and logic with which Hume raised problems about the justification of induction, we may raise problems about the justification of any kind of nondemonstrative inference. … As I argued … Hume's arguments are not peculiar to induction by enumeration or any other special kind of inductive inference; they apply with equal force to any inference whose conclusion can be false, even though it has true premises. Thus, it will not do to dismiss induction by enumeration on grounds of Hume's argument and then accept some other mode of nondemonstrative inference without even considering how Hume's arguments might apply to it." [Salmon 1966]

But Popper would not agree that he has claimed that any conclusions drawn in science are justified—at least not just like that. It is fairer to say that he simply indicates that the *logic* of it is deductive. But that *does* mean that the rationally justified part is deductive, and on these grounds, Salmon can still wage his attack. And Popper does take seriously the idea that his corroboration might be a kind of induction, being as it is also a kind of "appraisal", and thus bearing similarities with induction, including the use of probabilities (in Popper's case, highly falsifiable theories are *a priori* less probable) [Popper 1966, p. 265].

However, the quantified aspect of corroboration is not, for Popper, a degree of *justification* for the theory, but is rather a measure of how well tested the theory is. "The whole problem of the probability of hypotheses is misconceived. Instead of discussing the 'probability' of a hypothesis we should try to assess what tests, what trials, it has withstood… how far it has been corroborated [Popper, p. 215]." Now of course, how well tested it is is not ultimately just a logical matter, depending as it does on our *choices* of tests and hence background knowledge. The logical and hence justified part is in the "relations between the theory and the accepted basic statements [Popper 1959, p. 269]." So the number that comes out of corroboration is not really itself ultimately justified—Popper even gives alternate suggestions of how to compute the number. It is, however, related to the logical probability of the theory in question, although "only indirectly and loosely". Low prior probability makes a theory highly falsifiable and thus more highly corroborated when it passes attempts at falsification. Each such passing grade that the theory gets also increases corroboration, but less and less for each new test—unless the theory gets tested in a new way. Popper is the first to admit that there is no real, objective numerical value under all this. We are justified in our quantification scheme if it fits the general guidelines Popper lays out and is consistent with his vision of falsification. But it relates only "indirectly and loosely" with any real objective probabilities (the existence of which Popper does not deny) [Popper 1959, p. 269].

The quantified part of Popper's system is hence not the rationally justified part. "We cannot define a numerically calculable degree of corroboration, but can only speak roughly," says Popper [Popper 1959, p. 268]. So quantifications are possible, but the details are arbitrary to some extent.

So, since the quantified aspect of Popper's system is *not* the rationally justified part—in a sense it is the skeptical part—then its having a probabilistic nature something like induction may not be such a blow to Popper. On the other hand, that still leaves Salmon with his argument that the only rationally justified part of Popper's system is uninformative, and hence he is *still* some kind of inductivist to the extent that he is not a total skeptic.

However, I think Salmon is still not being completely fair to Popper, and is a bit guilty of reasoning from a false dichotomy—that is to say, he assumes that Popper must *either* be a total skeptic with respect to the justification of the knowledge science brings, *or* he must be an inductivist, since that is the only kind of justified system (presuming it *can* be justified) that adds new information. But these are by no means the only choices available to us. Popper's system is an interesting and subtle mix of deductive and nondeductive factors, interacting in a very specific way, and one cannot hold him to the artificial divisions of "justified" versus "unjustified" and "ampliative" versus "nonampliative", and the specific relations between them that are imposed by some other, completely different system, which is I think something like what Salmon is doing. Salmon is after limits of infinite sequences, and he is looking to "jump ahead to the end of infinity" so to speak, by "adding information" in the form of an inductive leap. Popper really does not believe this is what science does, and he divides the scientific process up in a completely different way than Salmon, emphasizing completely different things. Salmon is complaining that the only justified part of Popper's system is uninformative. The assumption he makes here—which is exactly the kind of "naïve empiricist" view that Popper is countering—is that it is the production of new information that must be justified in the first place.

Yet, to take Popper's perspective, this is in fact completely counter to the long tradition of Western empiricism! Empiricism is about starting with one's experience and sense data—which is just a given from the world—and *then* being rational in what we do with this. Why on Earth, Popper might counter to Salmon, would you insist that the *source* of our information about the world be *justified*? It is the *formation* of a theory or model *from* that information source that we wish to justify, and there is no inherent reason by way of information content or informativeness that we must justify the actual production of the new information—*that* comes from the world.

The problem is that there is also a Western empirical tradition, developed concurrently with the above, that *does* view the production of new information as justifiable, and that is the view of science as induction. But one could argue that this view is actually counter to the fundamental principles of empircism as laid down by early modern philosophers like Locke. (Of course, I am not looking to hold either Popper or Salmon to any particular view of empiricism, traditional or not, but I think they both assume an empiricist perspective, and so I think it relevant if they are following two conflicting ideas of this tradition.)

Popper *is* claiming however, that we cannot justify our confidence in surviving theories, yet we *can* quantify our confidence! This is still an odd position, and one still feels that if corroboration could be shown to be mathematically analogous to induction, that Salmon would have Popper in a corner. The problem is that we must look very carefully at the sources of the information content that both Popper and Salmon are always so concerned about. The information source for the degree of corroboration, as distinct from the information source for the elimination of theories, is completely determined by background information and other unjustified sources. So, while it is important that Popper's system provides the ability to quantify—since without that we would have no account for why our deductive justifications of theory elimination *does* seem to result in increased confidence in what is left. Popper is simply not claiming justification for this aspect of science, only an accounting of it. The justification is only for the eliminative aspect. So even if Salmon does show that corroboration can be reduced to induction, there is a sense in which this does not destroy Popper, since this aspect of the system is not the justified part of Popper's view in the first place.

Salmon believes he has shown that falsification and corroboration can be unified into a Bayesian probabilistic scheme, wherein falsification is simply the extreme case of a zero prior probability for an hypothesis, while degrees of corroboration come into play for intermediate prior probabilities. Falsifications of competing theories increase the new prior probability of the corroborated theory.

Whether or not this backs Popper into the inductivist corner, he certainly does not seem to believe that such a reduction is possible, and we certainly could say that if Salmon is successful at this, he will have shown that Popper did not take induction seriously enough. Popper even states that the use of probability in his system is exactly the *opposite* of its use in induction: "*the view implied by probability logic is the precise opposite of this [falsification view]*. Its upholders let the probability of a hypothesis increase in *direct proportion* to its logical probability—although there is no doubt that they *intend* their 'probability of a hypothesis' to stand for much the same thing … [as] 'degree of corroboration' [Popper 1959, p. 270]." So if Salmon could show that in fact the relation between Popper's use of probability and that in inductive logic is even qualitatively the same, he will have succeeded in showing that inductive logic is at least *closer* to what Popper is doing than Popper himself allowed.

At this point, Salmon's basic strategy should come as no surprise to you—he needs to show a connection between the probabilities used in a frequentist inductive logic, and the probabilities that determine the degree of corroboration in Popper's system. Popper claims this is the inverse of how probabilities are used in inductive logic, so Salmon must show he is mistaken about this.

Bayes' Theorem is the primary inference engine in any probabilistic inferencing system, including inductive logics, and Salmon takes the strategy of trying to show that falsification is one particular kind of application of Bayesian inferencing. There are several formulations of the theorem (for instance, Bayes' original formulation [Bayes 1763] was modified by LaPlace [1814]), but we will start with the following common variant:

$$P(H \mid E, C) = \frac{P(H \mid C) \times P(E \mid H, C)}{P(E \mid C)}$$

$P(H \mid E, C)$ – posterior probability of hypothesis H, given evidence E and background C
$P(H \mid C)$ – prior probability of hypothesis H, given background C
$P(E \mid H, C)$ – likelihood of the evidence if the hypothesis H is true, given background C
$P(E \mid C)$ – prior probability of the evidence, independent of the theory

Here, H is the hypothesis being tested, and E is the evidence. C is, like *c* in Salmon's justification of induction, a kind of "corrective term" representing, for instance, background information and metaphysical assumptions. It is sometimes called the "context" of the inference. (I have used a somewhat more conventional notation above than Salmon actually uses, so I will take the liberty of converting Salmon's expressions into the above.)

The formula is a calculation of posterior probability of an hypothesis or theory, and as such it performs an appraisal of a theory, just as Popper's corroboration measure does. For simplicity of exposition, we will ignore context for the time being (something pure induction does anyway!), and say that the posterior probability of a theory is based on the evidence, and is computed by multiplying the product of the priori probability of the theory by the likelihood of the evidence if the theory is true. This is then divided by the prior probability of the evidence, a quantity completely independent of our theory. It can be viewed as a scaling or normalizing factor, so that any mutually exclusive, exhaustive set of theories will have probabilities that sum to 1—as such, it is in a way a book-keeping measure, but an important one.

To illustrate this with a very simple example, assume we have a bag full of red and blue marbles, from which we will randomly pick one marble, return it to the bag, and repeat indefinitely. Now, there is some objective truth to the matter as to the "real" probability of picking a red marble, so this is a truth about which we can theorize (imagine it as a law of nature, if you will). The inductivist would suggest that we simply start picking out marbles, and keep track of the relative frequency of red ones. We adopt this tentative probability measure as our current best guess as to the real proportion of red marbles, and then update the guess with each pick. The guess should converge to the correct value in the limit of infinite picks. This is essentially the model of pure induction we saw from Salmon earlier. We are not employing any corrective measure or background context here, because Salmon's principle of invariance told us that this should be zero anyway, so we are better off using pure induction (of course, recall that this result is in question for the short run). In the contrived example of a marble pick, this method seems to be a correct one, and Salmon would like to show that we can also consider it a kind of falsification method.

Assume we have 3 red marbles and 7 blue marbles:

$P(\text{red}) = 3 / 10$

$P(\text{blue}) = 7 / 10$

The above constitutes the correct theory that we would like our scientific method to hit upon. However, before we actually do an experiment (a marble pick), we have no prior knowledge of what these values are. We also have no “direct access” to this information—only indirect access via marble picks. So how do we assign prior probabilities to a particular theory, say that P(red) is 0.7? We cannot, unless we have some context, or background knowledge that creates this *a priori* assumption in the first place. Bayesians allow us to assign these according to metaphysical, even purely emotional reasons, while non-Bayesians (“frequentists”) do not—any probabilities must reflect actual frequencies only. Both Popper and Salmon are frequentists, Salmon by virtue of his acceptance of pure induction with c=0, and Popper us too, even though he uses background knowledge (in effect having nonzero c), and in spite of the fact that he does not believe that corroboration is a probability analysis in the first place. This distinction is not really important for our immediate purposes, however, since Popper does not really think either the Bayesian or the frequentist views of probabilistic inference apply to corroboration. So we say for now that there is some prior probability, and leave it unsaid from whence it came.

Let us assume that our prior bias here is against the truth of the matter, and we believe that p(red)=0.7 is most likely the case, and we feel—for whatever reason—that we are about 60% sure of this. Since each value of P(red) completely determines the value of P(blue), it will simplify things to describe each candidate theory in terms of its value for P(red). So initially we have hypothesized P(red) = 0.7. Now say that we pick one marble and it is red. The prior probability of picking red once in one try given our hypothesis is, of course, 0.7. This result is a piece of evidence and we can now compute for the first time a probability for our theory by applying Bayes’ theorem:

$$P(H \mid E) = \frac{P(H) \times P(E \mid H)}{P(E)}$$

$$P(0.7 \mid red) = P(0.7) \times P(red \mid 0.7) \; / \; P(red)$$

$$P(0.7 \mid red) = 0.6 \times 0.7 \; / \; 0.5$$

$$P(0.7 \mid red) = 0.84$$

Since our first piece of evidence squared with our expectations (although not with the truth), the probability of our theory has gone up, from 70% to 84%. Now, we can incorporate our new "knowledge" into our prior probabilities. Say we pick a blue marble next:

$$P(0.7 \mid blue) = P(0.7) \times P(blue \mid 0.7) \; / \; P(blue)$$

$$P(0.7 \mid blue) = 0.84 \times 0.3 \; / \; 0.5$$

$$P(0.7 \mid blue) = 0.504$$

And another blue marble:

$$P(0.7 \mid blue) = P(0.7) \times P(blue \mid 0.7) \; / \; P(blue)$$

$$P(0.7 \mid blue) = 0.504 \times 0.3 \; / \; 0.5$$

$$P(0.7 \mid blue) = 0.3024$$

And now a red one again:

$$P(0.7 \mid red) = P(0.7) \times P(red \mid 0.7) \; / \; P(red)$$

$$P(0.7 \mid red) = 0.3024 \times 0.7 \; / \; 0.5$$

$$P(0.7 \mid red) = 0.42336$$

You get the idea. After our confidence went up with the first red pick, we had two blue picks, which drove it down from 84% to 30%. But then another red marble drove it back up to 42%. Note that we have now had an equal number of red and blue marbles picked, while we expected to get more reds, so our probability is lower at 42% than our initial prior probability was (at 60%).

As you can see, the short-run probability after a few picks does not mean much. But, due to the "uniform" nature of the actual marble bag, we can be sure that the posterior probability will converge on zero in the limit of infinite picks (since any pick that violates our expectation drives the probability down). Only for the hypothesis of P(red) = 0.3, the true value, will the limit be equal to 100%.

Now recall that Popper is adamant that his theory has nothing to do with any of this. Here, a high prior probability for the theory corresponds to a more highly confirmed theory (a higher posterior probability). But for Popper, a surviving theory that has a higher prior probability will be *less* well corroborated, not more so. So corroboration is not inductive. Or so says Popper.

Salmon is not convinced. "The hypothesis," he explains, "that runs this kind of risk of falsification without being falsified gains more in posterior probability than one that runs less of such risk. This does not mean, however, that the hypothesis itself must be implausible. A small value… [of P(E)] is perfectly compatible with a large value for… [P(H)] [Salmon 1966, p. 119]."

So a highly corroborated theory for Popper runs a great risk of falsification. This means, to Popper, that the *theory* had a low prior probability. But Salmon is suggesting that it does not mean that at all, but rather it means that the *evidence* we collected had a low prior probability (Bayes' Theorem can also be formulated so that this value is instead the probability of the evidence given that the theory is false, but the basic point remains the same). It is perfectly possible that the evidence that backs up our theory (fails to falsify it) is unlikely independent of our theory (or assuming it is false), while the theory itself has a high prior probability.

This may seem counter-intuitive at first, but is more plausible on closer inspection. The prior probability of the evidence (the normalizing factor) may be in a sense a book-keeping measure, but it is crucial here. Like many book-keeping measures, it provides a standard—here, it is the standard by which all our alternative theories are judged. It tells us the prior probability of getting a certain kind of result, regardless of theoretical considerations that would explain such an event in terms of some theory. Let us say that our theory is that there is a force of "gravity" or somesuch pulling things towards the Earth. Independent of the contribution of this theory, what is the probability that an object when place in front of us will start to mysteriously move towards another, distant and non-touching object? We might suppose that the probability would be low. Bringing in the theory changes things, however, since if the theory is true, the probability becomes higher.

One might argue, however, that the prior probability of the object accelerating towards the Earth must actually be high, since our theory's probability was high, and the theory implies this evidence. However, the alternative version of Bayes' theorem mentioned earlier—which is the one Salmon actually prefers—has a denominator that represents the prior probability of the evidence *given that the theory is false*. In this case, it is straightforward that such a probability, while no longer "independent" of the theory, can certainly be low while the prior probability of the theory is high (and both versions of the theorem amount really to the same thing mathematically, and both are just as appropriate for inferencing).

While Popper describes the low probability that implies high falsifiability as if it were the prior probability of a theory, Salmon suggests that what Popper *really* meant was the probability of evidence given that the theory is false, which is consistent with induction through Bayesian inferencing, via at least the second version of Bayes' theorem (described above).

Salmon even thinks he catches Popper wording it in this very way in *Conjectures and Refutations*:

> "A theory is tested not merely by applying it, or by trying it out, but by applying it to very special cases—cases for which it yields results different from those we should have expected without that theory, or in light of other theories. In other words, we try to select for our tests those crucial cases in which we should expect the theory to fail if it is not true." [Popper 1963, p. 112, In: Salmon 1966, p. 120]

Salmon believes this quote shows "how admirably his [Popper's] conception… fits the Bayesian schema." If we consider that these prior probabilities are all dependent on C, this Bayesian vision of Popper that Salmon is presenting is analogous to Popper's own notion of using background knowledge to determine tests, and then using the result of those tests (some degree of corroboration) as background for the next. It is this pragmatic justification of induction of Salmon's that can provide pragmatic background considerations affecting the whole process described in Popper!

If this is so, and Salmon presents a convincing case, then Popper's argument against induction becomes much weaker. It is still true that Popper is not necessarily tied to viewing his system as inductive, since his background information and prior probabilities (if we are to follow Salmon and call them that) are shoved off into the unjustified periphery of his method, away from the more solid foundation of *modus tollens*.

5. Conclusion

So in the last analysis, the two views may be formally compatible, and perhaps the difference is more one of emphasis—yet this difference in emphasis is perhaps the most important aspect of both these alternative views to begin with, rather than some technical, mathematical or logical difference. So let us for now allow, for argument's sake, that Popper might concede that corroboration is inductive (although he by no means would necessarily have conceded this point so readily!).

So the question is this: are we primarily doing deduction when we as rational scientists try to find a model to explain our short-run observations, or are we doing something that is fundamentally less certain, but in some sense just as rational? Popper believes the rationality of science lies in *modus tollens* and falsification, while the less certain "inductive" part is just due to the pragmatic decisions that must be made in order to actually apply this reasoning to the world. Salmon believes instead that this pragmatic judgement that we make is itself a rational process that can be rationally justified. He still believes that Popper's falsification is an accurate picture, however, and he does not deny that *modus tollens* is operative in that aspect of scientific testing.

But here we find the crucial difference between these two men. Salmon is seeking to place as much of science as he can within the scope of rationality. In that sense, he is displaying a rationalist faith in the uniformity of nature. Popper, as we have seen, has no such faith, but is more (as Salmon sometimes hints) of an empiricist-skeptic like Hume, accepting uniformity in nature only when he feels forced to, and even that is not the aspect of his science that he feels "justified" doing!

Salmon has shown well how Popper's system might be reframed as induction—with modest changes that may even be what Popper actually said in places. Popper, however, has still shown how it can be looked at as something totally noninductive. Which is the real "method" of science? It is a matter of emphasis and value. Do we assume uniformity—and uniformity of a certain type, at that? Or, do we remain skeptical with an eye to rational enquiry? Scientists themselves, for the time being, seem to recognize more in Popper of what they are doing in their laboratories. When they sit down to their work, it is primarily falsification that they are up to, not inductive generalization. Not that a popularity vote amongst scientists settles the issue! And while we cannot settle a philosophy of science argument by asking scientists, it would appear that we must at least look at scientists and study what they do in a real day-to-day pragmatic way, if we are to really settle the debate between Popper and Salmon. Is the scientific emphasis, tone and main thrust inductive and positive, or deductive and negative? It could be that both systems can be equivalent formally on paper, and still one may be far closer to how scientists really go about their work advancing our knowledge.

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